The biogeochemical−physical coupling of selenium and nutrients in tidal freshwaters of the SF Bay/Delta: an interdisciplinary field and modeling approach

Gregory A. Cutter

Public Comments

No public comments were received for this proposal.

Initial Selection Panel Review

Proposal Title

#0260: The biogeochemical−physical coupling of selenium and nutrients in tidal freshwaters of the SF Bay/Delta: an interdisciplinary field and modeling approach

Funding:

Do not fund

Initial Selection Panel (Primary) Review

Topic Areas

- Environmental Influences On Key Species And Ecosystems
- Relative Stresses On Key Fish Species
- Processes Controlling Delta Water Quality
- Water Management Models For Prediction, Optimization, And Strategic Assessments
- Assessment And Monitoring

Please describe the relevance and strategic importance of this proposal in the context of this PSP. How does the proposal address the topic areas identified above? What are the broader CALFED Goals this proposal may meet that are not accounted for in these specific topic areas?

Selenium is judged to be a crucial contaminant in determining the vitality of Bay−Delta ecosystems, broadly defined. How crucial is not clear, nor is it clear that this proposal will provide clarification to that question. It will, however, provide further information and a model which will assist managers in understanding how Se is transported through the Delta, which certainly will be valuable in short and long term management decision making.

The budgets of proposals submitted in response to this PSP are larger, on average, than those submitted to CALFED in previous years. The Science Program is committed to getting as much science per dollar as is reasonably possible. With this commitment in mind, can the proposed budget be streamlined? If so, please recommend and clearly justify a new budget

total in the space provided.

This is a very high price tag for a project within the Science Program, so it needs to be acclaimed as excellent. By and large that is the case, but a couple of the reviewers and the TSP express concerns about the size of the budget and whether the results will be of that much use to the Science Program and CALFED in general. On the other hand, it is difficult to know where to cut. Perhaps the best that could be done is to have a direct meeting with the research team, and determine how much "fat" there is in the budget, i.e. what could be cut and still achieve the primary outcome objectives.

Evaluation Summary And Rating.

Provide a brief explanation of your summary rating and any additional comments you feel are pertinent.

One of the reviewers notes that sampling is to be carried during low and high flow periods, and intensely, then asks: "is this the best way to understand this problem? I'm concerned about what happens during the rest of the year(s) when measurements are not taken. Are there sufficent measurements to guide model calibration, verification, etc for the remaining portions of the year. In short, what sort of variablility is to be expected? Are these measurement windows sufficient?" The reviewer also wonders if this choice of sampling frequency was due to the fact that getting such a large team assembled is difficult. That probably should be investigated, along with a close look at the budget to look for reasonable cuts.

Selection Panel (Discussion) Review

fund this amount: $**0**
note:
do not fund

The proposal is finely crafted and the project team has outstanding credentials. The panel recognized the potential

value of understanding the mechanisms that determine Se movement in the Estuary. However, the panel felt the proposal was too expensive for the value of the potential products. The proposed modeling and research products are scientifically interesting but will not necessarily address the important management decisions regarding selenium management in this system. The panel was concerned about the size of the budget for this proposal, aside from the value of potential products. For example, the modeling section of the proposed budget appears overly generous, although, without a deeper understanding of the work plan, it is difficult to know what could or should be cut, if anything.

Panel Ranking: Do not fund

Collaboration Panel Review

Proposal Title

#0260: The biogeochemical−physical coupling of selenium and nutrients in tidal freshwaters of the SF Bay/Delta: an interdisciplinary field and modeling approach

Final Panel Rating
above average

Collaboration Panel (Primary) Review

Collaboration:

Will the results of the collaborative effort be greater than the sum of its parts? Is it clear why the subprojects are part of a larger collaborative proposal rather than several independent smaller ones?

superior
large, complex set of activities linked closely, representing efforts of first−class investigators from several outstanding research centers

Interdependence And Integration:

Does the proposal have an example that clearly articulates the conceptual model of each subproject and how they link together as a whole? Are the boundaries of the study plans focused and cohesive, yet well delineated? Is there a plan for potential differences in the stages of subproject completion times? Are there clear plans for analyses and interpretations which seek to identify and quantify relationships among the data collected in various subprojects rather than separate analyses for each subproject?

superior
well thought out; the tasks and their interdependent linkages should yield a quantitative, integrated results

Project Management:

Is it clear who will be performing management tasks and administration of the project? Are

there resources set aside for project management and time given for investigators to collaborate? Is there a process for making decisions during the course of the project? Are there acknowledgments of potential barriers to collaboration and explanations of how team members will overcome barriers particular to their institutions?

superior
the plan is well laid out with a clear breakdown description; the lead investigator is very much the journeyman, and an authority on selenium

Team Composition:

Does the lead principal investigator have successful management history and experience leading collaborative teams? Is it clear that all key personnel are committed to making significant contributions to the project? Do team members have complementary skills?

superior
the team is well balanced and perfectly matched and should be successful;the lead investigator has a long and successful history in both collaborative research and in the subject matter; much of the team has worked together for a relatively long period of time

Communication Of Results:

Is there a clear plan for comprehensive and cohesive reporting of project progress to the CALFED community?

superior
the findings will reach the related Calfed projects as well as the general public

Additional Comments:

Collaboration Panel (Discussion) Review

Primary reviewer felt that the proposal well conceived written

in almost all categories; however, project management activities and budgets are not well described. The team has excellent reputations. Secondary reviewer is in agreement. Based also on past experience of the group and solid team dynamics, the propsal was rated Above Average.

Technical Synthesis Panel Review

Proposal Title

#0260: The biogeochemical−physical coupling of selenium and nutrients in tidal freshwaters of the SF Bay/Delta: an interdisciplinary field and modeling approach

Final Panel Rating
superior

Technical Synthesis Panel (Primary) Review

TSP Primary Reviewer's Evaluation Summary And Rating:

This is a very large multi−disciplinary and multi−institution proposal to study selenium as an anthropogenic enhancement in the Bay/Delta. It is lead by Greg Cutter, who is considered "Mr. Selenium", largely from his pioneering work in the Delta region starting in the 1980s. I would think that there would be no better person to head up such an ambitious effort. He has assembled an outstanding group including his expertise in selenium biogeochemistry and modeling, Steve Monismith (Stanford) and Mark Stacey (Berkeley) for physical transport and modeling, Adina Paytan (Stanford) for nutrient cycling and stable isotope water mass tracing, Nick Fisher (Stony Brook) for phytoplankton interactions, Tim Hollibaugh (Georgia) for heterotrophic bacterial interactions, and Bochdansky (at his own institution, Old Dominion) for zooplankton interations. A very ambitious project is proposed, but as pointed out by one of the external reviewers, "the strength of the PIs and the preliminary experiments that have been carried out suggest a good chance of success". Many aquatic environment problems require large scale broad efforts and many of them are doomed to failure by not having a really good and feasible plan. This proposal appears to be outstanding in design, personal, and plan. There is the often encountered problem of how can one do the extensive sampling and analyses required with adequate

representative sampling. This is addressed by the reviewers and I comment on it further below.

Additional Comments:

This is a very large multi−disciplinary and multi−institution proposal to study selenium as an anthropogenic enhancement in the Bay/Delta. It is lead by Greg Cutter, who is considered "Mr. Selenium", largely from his pioneering work in the Delta region starting in the 1980s. I would think that there would be no better person to head up such an ambitious effort. He has assembled an outstanding group including his expertise in selenium biogeochemistry and modeling, Steve Monismith (Stanford) and Mark Stacey (Berkeley) for physical transport and modeling, Adina Paytan (Stanford) for nutrient cycling and stable isotope water mass tracing, Nick Fisher (Stony Brook) for phytoplankton interactions, Tim Hollibaugh (Georgia) for heterotrophic bacterial interactions, and Bochdansky (at his own institution, Old Dominion) for zooplankton interations. A very ambitious project is proposed, but as pointed out by one of the external reviewers, "the strength of the PIs and the preliminary experiments that have been carried out suggest a good chance of success". Many aquatic environment problems require large scale broad efforts and many of them are doomed to failure by not having a really good and feasible plan. This proposal appears to be outstanding in design, personal, and plan. There is the often encountered problem of how can one do the extensive sampling and analyses required with adequate representative sampling. This is addressed by the reviewers and I comment on it further below.

Technical Synthesis Panel (Discussion) Review

TSP Observations, Findings And Recommendations:

The external technical reviewers (three with overall ratings of very good, very good, and excellent) and the panel agreed that this was a well prepared proposal with a capable and experienced research team, and that the project was very likely to contribute significantly to our understanding of the

topic. There was, however, concern regarding the sampling design for the physical transport experiment, and how well the resulting data would represent space and time variability. This may potentially limit the contribution of the proposed research to our understanding of selenium transport and fate throughout the Bay−Delta system (an important related question). The panel also questioned the size of the budget for the scope and extent of field work proposed.

Technical Review #1

proposal title: The biogeochemical−physical coupling of selenium and nutrients in tidal freshwaters of the SF Bay/Delta: an interdisciplinary field and modeling approach

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**The goals and objectives are clear and consistent. This proposal is all about developing a better understanding of selenium as it moves from external sources into and out of the upper portion of an estuarine system. The authors will use field and modeling studies to achieve objectives and all of this is appropriate**
Rating	**very good**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**The authors provide a clear picture of the selenium issue in the SJ and adjacent areas. The review (pgs 2−14) was helpful. One thing that was missing,however, was some data that clearly says selenium is a major problem. There were brief references to two fish species. It may be common knowledge that selenium causes mass mortalities, disrupts reproduction, causes large−scale larval mortalities, etc. But, that was not in this proposal. So, I am taking it on faith that this is a major issue. That case was not clearly made here. A table of documented selenium effects would**

	have been very helpful.
Rating	**good**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**The authors made a strong case for the difficulties in measuring processes in this heterogeneous environment. In some places physics mask many things while in areas with less active transport processes they have been able to see biological and chemical processes at work. These facts are reflected in the design of the work and that is to the good.** **The authors devoted a great deal of space to the details of biological rate and stock measurements; there was very little discussion of the physical models. They make the case that they have a model and that it works. Evidence presented indicates that this is largely true. They also, in the background section, stated that they know quite a few things, suspect others and are in the dark about still other features of this problem. This proposal takes that into account and tries to move the knowledge base forward.** **I had two major pronb;ems with the approach, both of which may be more apparent than real. First, the sampling regime...when most of the work will be done...is scheduled for high and low flow periods. Sampling is intense for 24−48**

hours with some features being measured for longer periods of time. But, the authors have chosen to make a lot of measurements during very short time periods. This may be the best way to organize this large team of scientists coming from many different institutions around the country. However, is that the best way to understand this problem? I'm concerned about what happens during the rest of the year(s) when measurements are not taken. Are there sufficent measurements to guide model calibration, verification, etc for the remaining portions of the year. In short, what sort of variablility is to be expected? Are these measurement windows sufficient? There was little justification for the approach adopted, at least in the proposal. However, the authors and funding agency may have the background information that makes this concern of small interest. Second, quite rightly there was a lot of detail concerning selenium chemistry and biology. In several places the authors stated that there were to be "parallel" measurements of C, N and P processes because of Se often travels with the biogeochemical cycles of these elements. There was very little about just what C, N and P processes will be measured other than primary production. No (or very little) mention of many of the processes associated with C, N, P biogeochemistry.

As written the progect seems difficult but feasible. Many involved have very substantial experience working in this environment and that is a very strong positive. Even with my concerns, there is little doubt in my mind that these investigators will find some interesting

	things about this area, about selenium, and about the probable impacts of restoration plans on Se transport to SF bay. I'm not at all convinced that they will have much definitive to say about the food webs that develop in the face of higher or lower Se contamination...but, that's not a central feature here.
Rating	**very good**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**As I indicated earlier there was a great deal of detail concerning technical approaches to measurements. In addition, many of those associated with this proposal have great experience in making these sorts of measurements in difficult environments such as the proposed study areas. These investigators are very experienced. So, they get high marks in this area.**
Rating	**excellent**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**They did not address this to any large extent. One of my previous comments had to do with getting appropriate information for most of the year when detailed measurements were not being made.**
Rating	**not applicable**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**Yes. The authors make the point that there will be a range of products including technical reports, open access data files, presentations at a range of meetings and publications in the technical literature. I think they make the case that at the end they will have a far better idea (quantitative idea) of the likely transport of various forms of Se from source areas to SF Bay. This includes a better understanding of internal losses and sources, relationships to seasonal conditions and some aspects of biological impact. The use of very detailed models seems appropriate because of the complex geometry of the areas rather than a more academic exercise of "can we do this".**
Rating	**very good**

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**The leadership in this proposal is a very experienced and productive group...they are grizzled veterans and it is very likey they will find some of the things expected, some things unexpected and a few things will fail or prove to be unimportant. They certainly are familiar with group efforts and know how to make them work. They will in all likeyhood**

	publish a great deal. Very stron here. Probably the strongest part of this proposal.
Rating	**excellent**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**The budget looks reasonable overall. My major concern is that the lead PIs will be devoting a relatively small amount of time to this project. I did not understand the note associated with Cutter's time involvement so that may be just my problem...he may be spending a large amount of time on this project and that would be appropriate....someone needs to stay focused on this complex assembledge of people and research components. This small allocation of time to any one project is not unique to this proposal...it is a general problem that will not disappear soon.**
Rating	**very good**

Overall

Provide a brief explanation of your summary rating.

Comments	**Overall rating of very good. Main concerns include:1) sampling schedule is very focused; is it adequate? 2) very little said about C, N and P measurements associated with Se measurements; 3) limited time committment of PIs. Major strengths include: 1) previous work on this issue and familiarity with study area; 2) very strong group of investigators**
Rating	**very good**

Technical Review #2

proposal title: The biogeochemical−physical coupling of selenium and nutrients in tidal freshwaters of the SF Bay/Delta: an interdisciplinary field and modeling approach

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**The PIs propose that the fate and transport of selenium through the delta is: (1) controlled by in situ biological reactions and physical transport; (2) analogous to, or coupled with, the cycling of major nutrients, and (3) critical to the transport of selenium from the delta to the estuary. They support this view with extensive data from earlier studies in the SF Bay estuary and preliminary experiments carried out in the delta. Their research on selenium is a high priority for the 2004 CALFED solicitation and their hypotheses are clearly stated.**
Rating	**excellent**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**The PIs provide an excellent overview of selenium cycling and what is and is not known about selenium biogeochemistry in the SF delta/bay region. This overview, based on their previous CALFED funding, forms the basis of the current proposal. While the PIs make claims in the introduction as to the potential impacts of selenium on higher trophic levels, they do**

	not follow up on this at the same level of detail as they do with the lower trophic levels. [NOTE: My rating of Excellent here is based on the assumption that the current proposal is not for the same research previously funded. I don't believe that this is so, but it is not possible to be sure.]
Rating	**very good**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**The PIs propose a challenging study to carry out intensive field research in a physically dominated environment. As difficult as it is, this is the type of work that needs to be done. This approach is strongly supported by the preliminary research and the significant track records of the PIs. While this research will likely produce new insights into selenium biogeochemistry, most importantly it should produce a robust model of selenium interactions and transport through the delta ecosystem. The strength of the proposal is the biogeochemical cycling of selenium through lower trophic levels.**
Rating	**excellent**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**This proposal is ambitious; however the strength of the PIs and the preliminary experiments that have been carried out suggest a good chance of success. There are**

	too few times when the large scale and ambitious interdisciplinary field programs that we often need to address critical issues are actually within our grasp. I believe that this proposal represents one of those opportunities.
Rating	**excellent**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**While this is not a monitoring program, significant information on bacterial, phytoplankton, and zooplankton biomass and production as well as the concentration of selenium and other major nutrient elements will be generated.**
Rating	**not applicable**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**The PIs note the importance of peer−reviewed scientific publication and CALFED technical reports. It seems to me that this project will produce enough synthetic information and modeling results that the PIs could also examine other mechanisms for transferring this information to the general public (e.g. interactive museum exhibits) who may now know of selenium by name but really do not understand its relevance.**
Rating	**very good**

Additional Comments

Comments	**This is a very well written and integrated proposal.**

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**This is one of the strongest interdisciplinary teams that I have seen. The past work of the PIs is relevant to the proposed study and the PIs have already displayed the ability to work together effectively.**
Rating	**excellent**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**Yes. While $2.2M is a significant figure, the personnel, travel, sampling and support costs for eight PIs is justified.**
Rating	**excellent**

Overall

Provide a brief explanation of your summary rating.

Comments	**This is a compelling proposal that is we thought out and justified.**
Rating	**excellent**

Technical Review #3

proposal title: The biogeochemical−physical coupling of selenium and nutrients in tidal freshwaters of the SF Bay/Delta: an interdisciplinary field and modeling approach

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**The goals are stated clearly, consistently and professionally. The research team seem to be very professional and competent in their approach and organization.** **The hypotheses are clearly stated. However, in large−scale descriptive and modeling studies such as these, hypotheses are very different from those presented for experimental studies. The investigators present very generalized hypotheses that are suitable for such field studies.**
Rating	**very good**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**The study seems justified in that it stands a good chance to elucidate selenium dynamics in a watershed and estuary where Se is a recognized problem. The proposed work is very ambitious, perhaps overly ambitious, but a full scale investigation as proposed is justified. The conceptual model is well explained. That is not surprizing because this is a follow−up**

	study to a previous selenium study by the investigators.
Rating	**very good**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**The proposed investigation is very complete. It utilizes the latest techniques for measurement of physical, chemical and biological characteristics of the river and estuary. That said, there is nothing novel about the investigation. Such studies have been done on the Chesapeake Bay, Potomac Estuary, and other estuary−river complexes around the world. It should yield new information about processes in San Francisco Bay/Delta. The U.S. Geological Survey has done many similar measurements in the Bay, but in a much more piecemeal fashion. The USGS also has generated some excellent physical models of the Bay that could be built upon for the purposes of this study. The integration of the selenium chemical and biological processes with existing physical models could help understanding of selenium dynamics in the Bay.**
Rating	**very good**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**The study certainly is feasible. As I said before, it is very ambitious, but doable. It is, by its nature, an expensive project, but field studies of large water bodies are inherently**

	expensive.
Rating	**very good**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**If the proposal needs improvement any place, it would be in the expansion of the explanation of the data analysis procedures. The stated methods are reasonable, but could be expanded upon. I was left with some questions about the details of the analysis as well as the modeling procedures. Will there be calibration and verification data sets?**
Rating	**good**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**The project has potential to generate a vast quantity of valuable data and peer reviewed publications. I would anticipate publications on hydrodynamics of estuaries; primary production, secondary production, assimilation and metabolism of selenium by autotrophs, zooplankton, fish; sediment−water column adsorption and dissolution of selenium; and other topics. The investigation should result in a large, valuable database.**
Rating	**very good**

Additional Comments

Comments	**N/A**

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**The authors seem to be well qualified. I judge this by their resumes as well as by reputation and prominance in the literature.**
Rating	**very good**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**Large, whole estuary projects are very expensive, as is this proposal. Leasing and use of boats, sophisticated monitoring systems, intensive on station and drifting data collection are costly. So, although high in cost, the expense can be justified. If I were involved in the funding, I would look carefully at the potential returns of a mojor study such as this against the composite return of several smaller, less ambitious projects.** **(CBDA Staff Note: Staff chose "Not Applicable" rating in order to complete the review because reviewer did not provide rating)**
Rating	**not applicable**

Overall

Provide a brief explanation of your summary rating.

Comments	

	The investigators are a capable group, proposing an ambitious, intensive and extensive study of selenium dynamics in SF Bay. Such studies like this have been done on major estuaries around the world with emphasis on nutrients, phytoplankton, and various hazardous materials, i.e., persistant organics. From these studies, much more is revealed that just the focused upon element or compound. The studies tend to elucidate more about the various physical, biological and chemical systems as well.
Rating	**very good**